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MANUSCRIPTS should be submitted to the Editor, Charles M. Burrill, RCA Laboratories, Princeton, New Jersey.

ABOUT THIS ISSUE

This issue begins with two contrasting expositions of successful research. The first, by MALCOLM H. HEBB with the collaboration of MILES J. MARTIN, both of the General Electric Company, emphasizes the importance of free inquiry; the second, by RALPH A. RICHARDSON of General Motors, emphasizes, on the other hand, the importance of research toward specific goals. Dr. Hebb received his Ph.D. in physics from Harvard in 1936 and joined G.E. in 1949. He is manager of the General Physics Research Department of the G.E. Research Laboratory at Schenectady. Dr. Martin is the Research Laboratories' Manager of Research Information. Ralph Richardson received his degree in mechanical engineering from the University of Minnesota and has been with General Motors ever since. He is now Head, Administrative Engineering Department, General Motors Research Laboratories.

The issue continues with a paper by RAY P. DINSMORE, Vice President, Research and Development, Goodyear Tire and Rubber Company, on the importance of human relations in research. Dr. Dinsmore writes from 44 years of experience, all with Goodyear, since graduating from Massachusetts Institute of Technology in chemical engineering in 1914.

Last is a paper describing methods of salary administration developed to meet the needs of the research organization of a large chemical company. It is by GEORGE L. ROYER, who is Administrative Assistant to the General Manager, Research Division, American Cyanamid Company. Dr. Royer has been with Calco, now American Cyanamid, since obtaining his Ph.D. in chemistry from Cornell University in 1932.

The foregoing four papers were presented at the Annual Meeting of the Industrial Research Institute at Colorado Springs last May.

FREE INQUIRY IN INDUSTRIAL RESEARCH*

MALCOLM H. HEBB in collaboration with MILES J. MARTIN
General Electric Research Laboratory, Schenectady, New York

Research is systematic inquiry into the unknown. The very fact that the detailed course of a scientific inquiry cannot be charted in advance of new discoveries implies a certain amount of freedom on the part of the investigator. As new opportunities for exploration are revealed, the researcher must choose which to pursue and which to disregard or to postpone. His decisions will be influenced by a number of considerations: e.g., his special interests, the importance of the new possibilities to the main objective of his investigation, and the estimated probability of productive results. To deny *all* freedom is to nullify research.

Our concern here is the place of free inquiry in industrial research. How much freedom is compatible with the aims of an industrial research program? What criteria should determine the degree of freedom given an industrial researcher? How can the principle of free inquiry be incorporated in the administration of industrial research?

Before embarking upon a discussion of the various aspects of our topic, let us define the essential terms as we shall use them. The first of these is *free inquiry* also variously called "permissive"

* Paper presented at the Annual Spring Meeting of the Industrial Research Institute, Colorado Springs, Colorado, May 18–21, 1958.

research or "uncommitted" research—as opposed to "controlled" or "directed" research. Free inquiry is the pursuit of knowledge in which the investigator is at liberty to select research projects in line with his fields of interest and to pursue his studies in accord with his own judgment. Consideration of this concept of free inquiry leads us directly to the concept of basic or fundamental research, which thrives on freedom of inquiry.

So far as we know, no one has yet given a definition of basic research that is universally accepted. However, certain essential characteristics of basic research are generally recognized. The essential feature in any definition of basic research is freedom from practical demands. It is *science-oriented,* as opposed to *product-oriented* research. Basic research seeks new knowledge without regard to technological needs. Thus, basic research is usually defined in terms of motivation—but *whose* motivation?

The individual scientist may be motivated to explore a certain field solely by his own interest and curiosity. The course of his exploration will be determined by his evaluation of the challenge that each new discovery presents. This is the essence of free inquiry. An industrial sponsor, however, is usually motivated by the expectation of deriving eventual benefit for the business. It is not to be expected, therefore, that an industrially sponsored research program will extend far beyond those specific areas that show promise of yielding results of practical value. Does this limitation allow opportunity for free inquiry? It does to the extent that the originality, initiative, and professional judgment of the individual scientist can contribute to the research program.

We have emphasized the fact that the first characteristic of basic research is freedom of inquiry. An equally important one is the uncertainty of results. Basic research is a gamble with nature. The course of the exploration cannot be predicted nor can the results be anticipated. Just as Columbus set forth to find a road to China and, instead, discovered a new world, so basic research often leads us far from our initial objectives and opens up new worlds of science. The very nature of basic research

demands a high degree of freedom of inquiry. Like the explorer who must adapt his course to his discoveries, the scientist engaged in basic research must be free to explore the unexpected opportunities that his studies reveal. Thus, it is impossible to separate basic research from free inquiry.

We must be careful to distinguish here between the role of basic research in industry and the place of free inquiry in industrial research. The former is an important and interesting subject and not unrelated to the latter, but we must limit ourselves to the subject in hand—*free inquiry in industrial research.*

By far the greater part of industrial research is *applied research*, which, in broad terms, is the pursuit of knowledge bearing directly upon a recognized need. It is primarily *product-oriented.* Thus, applied research is more closely linked to current technology than is basic research; hence, it is easier to identify tangible goals. Consequently, the course is more definitely set and freedom of inquiry may be circumscribed to some extent by practical considerations.

We shall not dwell further upon the distinction between basic and applied research. Although it is possible to write down loose definitions of basic and applied research that are generally acceptable in principle, we invariably run into trouble when we attempt to use them to classify a given research activity. We soon discover that the borderline is not well defined and that the distinction is fraught with semantic difficulties. Rather than to adopt criteria for basic and applied research as our frame of reference, we shall endeavor to analyze the role of free inquiry throughout the broad gamut of industrial research with which we are all familiar.

Objectives of Research in Industry

We believe it would be well, at this point, to set down as clearly as possible the objectives of research in industry. It is in

the light of these objectives that we must view the role of free inquiry.

We believe that there can be little disagreement about the ultimate objective of industrial research. It is generally recognized that industrial research is undertaken in the hope and the expectation that it will lead to new or improved products or services that can become a part of the business or to new or improved processes that can be used in providing those products or services. Even where the research is of the most basic nature, it is generally motivated by the conviction that it can have a significant impact on the sponsor's business.

The attainment of this objective involves either the opening of new fields through the discovery of new principles or new phenomena or the extension of established fields through renewed interest in old ideas, frequently accompanied by the development of more refined and definitive concepts. The objective, as we have stated it, is sufficiently broad to encompass all of the aims of industrial research. However, it is useful to state explicitly some secondary objectives.

First, there is the general objective of assessing the potentialities and limitations inherent in a particular area of science and technology. This is an important benefit available from research which can be used to guide future plans and developments of the business.

Another aspect of research aimed at evaluating scientific or technological potential has to do with the exploration of apparently unpromising fields as insurance against surprise by an unexpected discovery that might obsolete existing technology. Although the scientific expectations may be small, the risks involved in ignoring even remote possibilities may be very great. The objective of this kind of research is somewhat similar to the familiar objective of "preventive maintenance" in a factory; the pay-off comes in guarding against a catastrophe in the form of a revolutionary discovery by your competitor.

There are good examples in the generation of electric energy.

At present, most electric power is produced by rotating machinery driven by steam turbines whose energy is derived from the burning of coal. Potentially, the whole system may be replaced by a fuel cell which converts the chemical energy of the fuel directly to electrical energy; the turbine-generator may be replaced by a thermoelectric or thermionic generator that produces electricity from heat without mechanical intermediates; or, finally, the fossil fuels may be replaced by nuclear fuels which can be utilized by any one of several generators. Both the power companies and the manufacturers of generating equipment will wish to explore the potentialities of these systems so that they will receive adequate warning of the demise of present technology.

In a sense, the opportunity that research affords in any area is the product of the likelihood of finding a significant research result and the impact that result would have on the sponsor's business. We use the mathematical term "product" hesitantly, since none of the variables involved is capable of the precise specification of measured physical quantities. About all that can be said is that there is a greater technological opportunity if either the chance of success is improved or if the potential impact of a successful result is increased. In particular, the opportunity is zero if either of these factors is zero. The dilemma comes when the research problem is very difficult and the chance of success very small, but when the probable impact of the accomplishment would be tremendous. As a rather extreme example, one might grant that research in general relativity *could* lead to a means of controlling, shielding, or nullifying gravity. Probably the most enthusiastic would regard "anti-gravity" as a remote eventuality; yet, the impact of "anti-gravity" on air transport, on airplane manufacture, and on space travel would be incalculable. Fortunately, or unfortunately, most areas of interest for industrial research have neither the inordinate potential impact nor the meager prospect for success of this example.

Another secondary function of an industrial research program is consultation—a broad-based activity whereby specialized

scientific knowledge is made available to development engineers in operating components of the organization. This is a very important aspect of most industrial research.

Finally, there is the prestige contributed by an effective research program to a business that capitalizes upon innovations created through its research program. The prestige value of an industrial research program depends on its impact upon commercial operations. Obviously, a research program that does not make a worthwhile contribution to the objectives of the business has questionable prestige value and certainly its use as "window dressing" is ill-advised.

Free inquiry in industrial research must be examined in the light of these objectives.

Approach to Research

There are two idealized approaches to the research problem which lie at opposite extremes. On the one hand, research may be carried out on a strictly *programmatic* basis, in which all of the effort is aimed directly at the specific requirements of the business. This approach affords the least opportunity for free inquiry. It is strictly product-oriented and, hence, is very effective in leading to products and product improvements that require no radical innovation. It tends to concentrate on those projects whose opportunity is good because the likelihood of success is high, even though the impact may be modest. In this approach, the horizons of tomorrow are limited by the viewpoint of today.

At the opposite extreme, the research objective may be sought through an entirely unprogramed activity of undirected research carried out in the hope that the results may somehow prove useful to the business. Such an extreme approach is the acme of free inquiry. It neither solves immediate problems nor assures the future, since it does not take into account the anticipated course of the business. It *will* offer unexpected results. Although

this approach allows the maximum freedom for the individual research worker, it suffers from a lack of perspective and an undefined scope.

The practical course lies somewhere between these two extremes. The actual research program in industry will attempt to achieve a balance, encouraging free inquiry in order to foster new ideas and results, yet guiding and directing to assure adequate coverage in specific areas. The balance between organized investigation and free inquiry will vary widely among industrial laboratories, depending on the needs and opportunities of the business. As a matter of fact, emphasis may shift from time to time in response to the demands of necessity or to the appeal of special opportunity.

Since our topic is free inquiry, we shall not dwell on the advantages and disadvantages of strictly programed research. It is well to remember that free inquiry in the tradition of the great universities of the world has provided business and industry with many important technologies. We need enumerate only a few instances: In 1887, Heinrich Hertz discovered electromagnetic waves, the basic means of transmission for radio communication, television, and radar. In 1895, Wilhelm Roentgen discovered new penetrating radiations which he called x-rays and which today are basic to a large technology, both medical and industrial. In 1887, Heinrich Hertz found that electrons were emitted when light fell on a surface. Today, the photoelectric effect is the basis of the cameras used in television and the sound pickup in movies. In 1938, Lise Meitner discovered the fission of uranium and opened the way for the nuclear power plants of the present and the future. Imagine the void in our technology that would result if these discoveries and the subsequent developments based on them were withdrawn.

We have said that the actual approach to industrial research will lie between the two extremes of strict programing and complete freedom. You may ask how, then, is free inquiry to be circumscribed.

In the first place, industrial research is a mixture. In any industrial laboratory, there will be a balance between free inquiry and programmatic research.

Second, freedom of inquiry will usually be limited to certain fields of research within the general interest of the sponsor. For example, an oil company might support research in geophysics but not physical electronics, or an electronics company might support work in solid state physics but not organic chemistry. The scientist will not see much limitation here, since his own inclinations are likely to be confined within one such broad field. However, for the sponsor, the limitation of field is important because it improves his chances of being able to utilize a successful research result.

Third, free inquiry will be the most fruitful approach only for a fraction of research workers. For others, closer guidance and a more planned program will be more effective. One can put the statement the other way around: for the kind of research that thrives on free inquiry, the problem will usually be to find individuals with the requisite initiative, competence, and originality to pursue research with full freedom. A suggestion of this is found in a statement of Dr. William D. Coolidge, former director of the General Electric Research Laboratory. He said that scientists in the laboratory were given all the freedom that they knew how to use.

The fourth consideration relates to leadership. This is not a limitation in the usual sense of the word, but represents an influence that expedites progress of the research and enhances the value of research findings. Even where an individual scientist has freedom in the pursuit of research, the direction that his work will follow and the rate at which it will advance, as well as the nature of the results that it will generate, can all be influenced without pressure or coercion by outstanding research leadership. Thus, while maximum incentive can be given to the exercise of his originality, some guidance to his thinking can come from the counsel of scientific authority.

Balance of Free and Directed Research

The proper balance between free inquiry and programed research is almost as difficult to determine as the amount of research that a company should do. The answer must be found by each company on the basis of its own particular situation.

Broadly speaking, the factors affecting the balance are the general and specific aims of the business. Plans to strengthen a particular process or product line may dictate an oriented program to that end. The purpose of making broad innovation or of opening a new field of technology may be better served by research relying heavily on free inquiry.

The type of industry and the nature of the business that the company has, or expects to have, are important considerations. A business that is broad in scope may stand to benefit fully from the diversity of results of free inquiry, while one that is narrow may be able to utilize only a fraction of the findings. An undiversified business may eschew free inquiry for this reason, yet indulge successfully in controlled research.

Another pertinent factor that influences the desired amount of free inquiry is the existing state of the technology upon which the business is based. In the case of an old and established technology, the possible sources of innovation are pretty well defined, and the promising courses of investigation are well charted. Although some opportunity for freedom of inquiry still remains and a certain amount of undirected effort is highly desirable, the dependence upon free inquiry will be less than it is in a field involving a new and incompletely developed technology. For example, in such a rapidly unfolding field as semiconductors, progress depends vitally upon free and unhampered investigation. *In opening up virgin territory, to deny freedom of inquiry is to slam the door on discovery.*

The specific functions to be performed by a research program will vary, but, in general, an industrial research activity serves its sponsor in two ways: (*1*) by making direct contributions to the

technology underlying the company's business, and (*2*) by providing a coupling with world science and technology—a contact that allows the progressive company to draw upon a vast inventory of technical knowledge. This second general function—that of providing a means of assimilating new ideas from all available sources and adapting them to specific situations—is a vital aspect of all industrial research activities, even those that are strongly product-oriented. Scientists are better able to maintain contact with world science if they are themselves engaged in research programs with a significant component of free inquiry. For some companies, this is the principal justification for encouraging free inquiry.

The appropriate balance of free inquiry and directed research is influenced by the relative effort devoted to short-term and long-term projects in a research program. Short-term projects are usually dictated by current need and, while the opportunity for free inquiry is not wholly lacking, the factor of immediacy places considerable restriction on it. Long-term projects will be less affected by urgency and will allow greater room for free inquiry than those demanding prompt results. Projects that are long-range often involve extensive search for new knowledge and the correlation of knowledge from several fields of science. A radical degree of innovation may be contemplated. It may then be best to rely heavily on the initiative, originality, and judgment of an inquiring scientist who is allotted a generous portion of freedom.

The most important factor in determining the amount of free inquiry has been left to the last. It relates to the growth plans of the business. A company that regards itself as stabilized at a fixed level in a traditional industry is likely to depend on conventional research—if any at all—directed along recognized lines. In a static business, research itself is not a live issue. One can inquire which is cause and which is effect.

On the other hand, a live enterprise in an industry with a potential for growth will have a calculated plan for that business

growth which depends upon technological advances. Such an enterprise can achieve its objective only through a coordinated research program aimed at innovation. The biggest factor in growth is innovation, and the biggest factor in radical innovation is free inquiry.

The growth of technology is somewhat like the growth of a tree. You can't tell where the next twig will form, and you can't guess where the next technological innovation will come. Therefore, you have to cultivate the whole field, a task that demands considerable freedom for the gardener.

Organization and Staffing

Research scientists working on projects of relatively long range and having substantial freedom of inquiry need a certain isolation from the current problems of the business. Otherwise, they tend to get drawn into the day-to-day activities. This may benefit the present, but it sacrifices the future.

At the same time, the isolation should not be complete. Large industrial organizations usually have centralized research facilities in order to serve the corporation from a broader perspective than is possible in the decentralized research activities with their limited and specific responsibilities. The problem then becomes one of providing adequate two-way communication between the central research activity and the decentralized operations. On the one hand, this fosters prompt and full use of research results and, on the other, it tends to insure that the research does not lose sight of important business objectives.

It is obviously desirable for the sponsor of industrial research to benefit by applying results of that research in his business. It is less commonly recognized that the utilization of research results is as important to the scientist. The thoughtful scientist will recognize that continuing sponsorship of research will depend on the company's ability to exploit its findings. He will not wish to see research results wasted when they could be made useful.

His morale will be raised and his appreciation of the objectives of the business enhanced when he sees his research lead to successful application in the organization. An industrial research program should not be instituted unless its sponsor is firmly committed to put its successful results into practice and has the plans and organization to do so.

Organizational flexibility is desirable for free inquiry in research. It must be easy to set up new projects quickly, to carry out preliminary investigations, and then to concentrate activity where opportunity has been disclosed.

Of paramount importance to free inquiry in industrial research is a true understanding on the part of the management of the company. A good atmosphere for free inquiry demands that management be kept informed of the opportunities for research, the capabilities and limitations of research, and the accomplishments of research. Only then will management be able to assess properly the place of research and of free inquiry in the enterprise.

The precept for staffing is simple: find the very best people, people with the most imagination and originality, the most creative people. Shockley has estimated that the most creative 5% of scientists generate 30 to 40% of original ideas. Really creative and original men are rare and priceless. Their creativity should be fostered by an atmosphere of motivation and stimulation that will bring out their best.

The leaders of research are particularly important. It is they, largely, who establish the atmosphere of research, who provide the inspiration and the guidance that are necessary for the most effective utilization of free inquiry in industrial research. Leadership must be recognized and developed wherever it is found. The best scientists without leadership are likely to make only indifferent contributions in industrial research, and poor leadership will jeopardize even those. The head of an organization, whether he is the president of a company, the director of research, or the leader of a research group, tends to set its character In general, *an organization will be no better than the man at the top*.

Impact of Free Inquiry

The best way to examine the impact of free inquiry in industrial research may be to look at some examples. The ones that we have chosen are not necessarily the best and are not representative of all industry. These are cases that we happen to know about. In several instances, they are taken from General Electric research.

Let us look at the commonplace incandescent lamp. Every schoolboy knows that the original electric lamp was a product of the inventive genius of Thomas Edison. But few people realize the long history of scientific research that lies back of the modern electric lamp. The gas filling and the coiled filament go back almost fifty years to the research by Irving Langmuir that was aimed at understanding evaporation and heat loss from hot filaments. Langmuir's starting point had been the observation that the bulbs of the old, evacuated tungsten-filament lamps slowly blackened with use. However, his curiosity carried him far afield from this immediate problem. Here are his own words:

"During these first few years, while I was thus having such a good time satisfying my curiosity and publishing scientific papers on chemical reactions at low pressures, I frequently wondered whether it was fair that I should spend my whole time in an industrial organization on such purely scientific work, for I confess I didn't see what applications could be made of it, nor did I even have any applications in mind. Several times I talked the matter over with Dr. Whitney, saying that I could not tell where this work was going to lead us. He replied that it was not necessary, as far as he was concerned, that it should lead anywhere. He would like to see me continue working along any fundamental lines that would give us more information in regard to the phenomena taking place in incandescent lamps, and that I should feel myself perfectly free to go ahead on any such lines that seemed of interest to me. For nearly three years I worked in this way with several assistants before any real application was made of any of my work. In adopting this broad-minded attitude, Dr. Whitney

showed himself to be a real pioneer in the new type of modern industrial research."

We might add that Whitney also showed himself to be a judge of scientific talent. Clearly, such complete liberty could not be accorded everyone, but with truly creative individuals, productivity is proportional to the degree of freedom.

The results of importance to incandescent lamps that came out of Langmuir's work can be summarized in three findings: (*1*) The bulb blackening in well made lamps was not due to residual gas as was commonly believed. It was simply the result of evaporation of the filament. (*2*) The rate of evaporation of the filament was considerably reduced by filling the bulb with an inert gas, such as nitrogen. However, a poorer lamp was the result, since the cooling of the filament by the nitrogen lowered the efficiency. (*3*) The loss of heat was greatly reduced by coiling the filament. Thus, the combination of the gas filling and the coiled filament produced a lamp of significantly higher efficiency.

A second good example of the fruitfulness of free inquiry in industrial research is afforded by the silicones. Building upon pioneer researches by the British chemist, Kipping, at University College, Nottingham, research chemists at the General Electric Company and at Corning Glass Works concurrently carried out extensive studies in the chemistry of the silicones. The silicones, as you know, are compounds involving chains of silicon and oxygen atoms that bear a structural resemblance to the hydrocarbons. The work at the General Electric Research Laboratory included the efforts of a number of chemists of differing interests and fields of specialization. No one knew what compounds could be made, what their properties would be, and still less what applications they would have. A great deal of freedom of inquiry was accorded the individual workers, and progress depended upon the unfettered initiative of the specialists in the pursuit of challenging opportunities. The result was progress along several fronts. Thus, one group, under the leadership of Dr. Eugene Rochow, discovered a simpler substitute for the complicated and costly

Grignard reaction which had formed the basis of previous silicone synthesis. Another group discovered the silicone oils and developed a method for producing them. Still another group made the first silicone rubber. Because this research was not rigidly programed, progress was not limited to specific foreseen objectives, but spread out and opened up a wide field of subsequent business opportunity. Today, silicones are essential to many products from electric irons to jet engines, and their sale in 1957 has been estimated to have been in the vicinity of $50 million.

The next case of free inquiry that we shall examine is in the field of semiconduction—the research by Bardeen and Brattain at the Bell Telephone Laboratories that led to the transistor. Bardeen and Brattain shared the Nobel Prize for 1956 with Shockley who directed the research. For perspective, it may be recalled that a large amount of research on the semiconductors germanium and silicon was carried out during the war in various laboratories in the United States and Europe. The primary interest was in point-contact rectifiers for radar. After the war, a more general investigation of the properties of semiconductors was undertaken at the Bell Laboratories. The following paragraph, written by Bardeen and Brattain in 1948, recounts the steps leading to the discovery of transistor action.

"The general research program leading to the transistor was initiated and directed by W. Shockley. Work on germanium and silicon was emphasized because they are simpler to understand than most other semiconductors. One of the investigations undertaken was the study of the modulation of conductance of a thin film of semiconductor by an electric field applied by an electrode insulated from the film. If, for example, the film is made one plate of a parallel plate condenser, a charge is induced on the surface. If the individual charges which make up the induced charge are mobile, the conductance of the film will depend on the voltage applied to the condenser. The first experiments performed to measure this effect indicated that most of the induced charge was not mobile. This result, taken along with other

unexplained phenomena, such as the small contact potential difference between n- and p-type silicon and the independence of the rectifying properties of the point-contact rectifier on the work function of the metal point, led one of the authors to an explanation in terms of surface states. This work led to the concept that space-charge barrier layers may be present at the free surfaces of semiconductors such as germanium and silicon, independent of a metal contact. Two experiments immediately suggested were to measure the dependence of contact potential on impurity concentration, and to measure the change of contact potential on illuminating the surface with light. Both of these experiments were successful and confirmed the theory. It was while studying the latter effect with a silicon surface immersed in a liquid that it was found that the density of surface charges and the field in the space-charge region could be varied by applying a potential across an electrolyte in contact with the silicon surface. While studying the effect of field applied by an electrolyte on the current-voltage characteristic of a high back-voltage germanium rectifier, the authors were led to the concept that a portion of the current was being carried by holes flowing near the surface. Upon replacing the electrolyte with a metal contact, transistor action was discovered."

Everything in the foregoing paragraph may not be clear to you, but it should be abundantly clear that this research was not and could not have been programed. As to its impact, you are all familiar with many applications of transistors. The business in transistors in 1957—the ninth year after their first announcement—was $70 million.

Our final example of free inquiry in industrial research is one that is still in a fairly early stage so that no *complete* story can be told. It has been known for many years—for as many years as thermionic emission, work functions, and contact potentials have been understood—that these phenomena offered a means of converting heat into electrical energy. Yet there has been little interest in the possibility, since most scientists have been convinced tha in-

surmountable limitations on efficiency and power output stood in the way of practical utilization. Some time ago, Dr. Volney C. Wilson in our laboratory came to the conclusion that research in this area might be rewarding, and he embarked on an experimental program. His previous experience had been in quite different fields: cosmic rays, radar, nuclear reactors. At the time of which we are writing, he had been engaged for a number of years in research on the magnetic properites of ferrites. As a man of demonstrated technical competence and experimental skill, he was accorded freedom to carry out his ideas. His success in this program was announced publicly by General Electric in November of 1957, at which time an efficiency of 8% had been achieved. What further progress can be expected and what practical application will follow are not yet clear. It *is* clear that a field of potential importance has been opened by free inquiry.

The four examples chosen to illustrate free inquiry in industrial research have ranged over some fifty years. The gas-filled lamp has received the judgment of history. Silicone chemistry and transistor physics have established themselves as the foundations of flourishing businesses, but their full measure is still to be established. Thermionic generation of power is a prospect for the future.

Fifty years ago industrial research was a rarity, and free inquiry was largely confined to universities. Industry depended on universities for the scientific findings that would lead to radical innovation. Today industrial research and free inquiry are accepted by enlightened companies, and industry is much less dependent on outside sources for scientific results. The trend will continue. Free inquiry in industrial research will grow.

RESEARCH TOWARD SPECIFIC GOALS: DEVELOPMENT OF THE LIGHT-WEIGHT, TWO-CYCLE DIESEL*

RALPH A. RICHARDSON

Head, Administrative Engineering Department, Research Laboratories, General Motors Corporation, Detroit, Michigan

A large percentage of the effort of the General Motors Research Laboratories has been aimed at specific goals. However, it must be stated that our efforts are not confined to problems of this nature. We have always supported work on many projects without specific objectives where the aim was to expand our knowledge in fields deemed pertinent to our business. In the past several years the trend has been toward increasing emphasis on basic or fundamental research in the physical sciences. Even on projects where the general objective is specified, research personnel are given full responsibility and authority for selecting the methods used in accomplishing that objective. In most cases a time limit is not imposed. This is particularly true of long-range exploratory projects where the aim is obtaining basic information necessary for executive decisions. Each project is closely followed, and frequent reappraisals are made as new information is obtained. The course of the development and the amount of effort allocated to it are determined by the results.

I have chosen to elucidate my subject of research toward specific goals by presenting a case history of the development of

* Paper presented at the Annual Spring Meeting of the Industrial Research Institute, Colorado Springs, Colorado, May 18–21, 1958.

our two-cycle diesel engine. This work was done long enough ago so that we are able to follow the steps from research to the end product and evaluate the entire development. It started with the specific goal of developing a new diesel engine, but was general enough so we were not restricted to any one size or application. It had facets of both basic engineering research and long-range development. It is an excellent case history for our purpose, since it shows how industry grows as a result of industrial research development.

Before we examine the steps and methods used in this project, it will be helpful to describe the organization and broad policies under which our Research Laboratories operates. General Motors is a decentralized organization in which each production division is wholly responsible for its own product development, as well as manufacturing, sales, service, and procurement. Each manufacturing division has its own engineering, metallurgical, and chemical departments. These departments have complete responsibility for their division's product and process development.

The Research Laboratories likewise operates under the policy of decentralized operations and responsibilities with coordinated control. It is one of several central-office groups organized to provide assistance to the divisions. The other groups at our Technical Center, which is located just outside of Detroit, are the Engineering, Process Development, and Styling Staffs. As a rule, central-office groups work on problems common to several divisions and operate central technical service facilities. Each has a budget to carry on its program but also may do work at the request of a division—at the expense of that division.

Central-office staff activities have neither authority over divisions nor responsibility for their final products. The divisions are free to use Research Laboratories developments and services as they see fit, but the choice to use or not to use rests with the division management. Our Research Laboratories provides only a very small part of the entire research, development, and engi-

neering conducted by General Motors. In fact, we make up less than 10% of the total.

The General Motors Research Laboratories was established about 40 years ago as an independent group to work on long-range fundamental projects free from the pressures of new-model development and current service problems. It was physically separated from the production engineering departments, with its own vice president of General Motors in charge. Research Laboratories is free to choose activities which its own management feels will be the most productive for the whole of General Motors five years and more in the future. This means that our program is composed of projects with an end application in view and those in which the extension of knowledge in a particular field is the only aim. Our research extends from such basic subjects as magnetic domains in single crystals of iron to long-range engineering studies of new forms of power plants.

Now, to return to the development of the two-cycle diesel engine, the initial aim of the project was a complete review of compression-ignition, heavy-oil engines to determine their future possibilities in our business. A second aim was the application of all our experience on light-weight, high-speed, internal-combustion, automotive engines to the development of a new type of diesel. The incentive to start this study was the inherent high efficiency of the diesel cycle which operated at compression ratios as high as 16 to 1 in comparison with 5 to 1 for the spark-ignition gasoline engines of that time. Since General Motors is basically a producer of power plants, it was natural to explore other types of engines which could become the products of the future. At the beginning of the work we were interested in the basic advantages of compression-ignition engines with little concern for size and application. This allowed the Research Laboratories to consider engines of many cylinder sizes without restriction to a single type. The aim was to develop a basically good engine with the knowledge that the possible applications would present themselves later. The project started as an outgrowth of our interest in obtaining

higher economy from internal combustion engines of all types, and was built upon our previous work on gasoline engines which had already given us higher power and economy for automobiles. It was also aided by our basic fuel study which had resulted in the discovery of the effect of tetraethyl lead as a knock suppressor.

From the very beginning, Alfred P. Sloan, Jr., who was then President of the General Motors, was personally very interested in the possibilities of the diesel engine. In a letter to Mr. Sloan, Charles F. Kettering, then Vice President in charge of Research, said: "I think we have a wonderful opportunity of going out and doing a real job here. The whole internal combustion engine problem has opened up in such a way that I feel we should be the leaders of every phase of it." Mr. Kettering felt confident that our study would be successful, since our research group had a wealth of experience in fundamental fuels and combustion studies, hydraulics, stress analysis, metallurgy, and high-speed engine balancing. In fact, our high-speed flame photography and other fundamental studies of combustion in spark-ignition engines had given us an understanding of engine fuel knock and the problems of combustion which was invaluable in obtaining an early understanding of the problems of compression ignition.

At the time this project was started, diesels were large, heavy, slow-speed engines limited largely to marine and stationary power-plant applications. These engines weighed as much as 200 pounds per horsepower and could attain maximum speeds of 150 to 600 rpm. In comparison, automobile engines weighed less than 10 pounds per horsepower and were capable of maximum speeds of over 3,000 rpm. The few previous attempts to build small diesel engines had resulted in rough running, smoky exhaust, and unreliable operation with low power outputs.

Up to the time of our entry into the diesel field, there had been very little research on diesel engines in this country. It is true that the National Advisory Committee for Aeronautics had made the first effort at research applied to diesel engines by setting

up a laboratory to make high-speed fuel-spray photographs. This had resulted in a better understanding of fuel injection.

Our program started with a thorough study of the present state of knowledge on oil engines. This was in accordance with generally accepted practice in inaugurating a new research project. I believe the value of such an initial study may easily be forgotten after a project is successful. Even in the research laboratory, we often give too little credit to reports and other paper studies. This study was a thorough review of compression-ignition engines. It resulted in two large reports. The first had the formidable title, "Classification and Condensed Survey of Heavy-Oil Engines, Especially High-Speed Engines for Automotive Use." The second was titled, "A Critical Review of Existing High-Speed Oil Engines and Some Important Slow-Speed Types." These reports contain an analysis of various types of engines and the problems to be solved in the development of high-speed engines for automotive use.

Even before the paper study was made, dynamometer testing of several diesels and fuel injection systems was under way as part of an extensive long-range program of internal-combustion engine development. Of 27 projects in the Power Plant Section, only one pertained to diesel engines. At this time, we had equally important projects on many other new engine types and engine components. It was Mr. Kettering's feeling that we learn best by doing, and he was a leading proponent of the experimental approach. He had even acquired a diesel-powered yacht so that he might obtain firsthand experience in diesel operation.

This first period of the initial growth of interest in the diesel engine covered about two or three years. Out of this preliminary study and laboratory work, management obtained firsthand information on the potential of diesel engines.

It is interesting to know what research men think during the early stages of a project. I have looked up the log book of F. G. Shoemaker, then assistant head of the Power Plant Section, for reference to his "on the spot" observations. Here is his note written before the work had made much progress:

". . . it is apparent that the G. M. Research organization is in a favorable position to carry on an investigation of the problems of increasing the speed and M.E.P. of oil engines in spite of our lack of previous experience with existing types of engines. The problems to be solved are the mechanics of the injection system, the processes of combustion, and the design of a light, high-speed engine structure. Our experience with high-speed valve gear and such instruments as the electrical stroboscope places us in a position to attack the injection problem in a way that is not likely to be found in the existing diesel-engine industry.

"Our long experience and study of fuels and combustion is not equalled by any of the diesel-engine producers. The fact that a large number of the engineers in Research have had aircraft engine experience, and their familiarity with engines of high rotative speed gives us a background of actual light-weight, high-speed, design experience that is well in advance of that of the engineers who have grown up in the diesel-engine business."

The first complete multicylinder engine built by our Research Laboratories was a small, three-cylinder, two-cycle engine which was installed in a Chevrolet truck. It was designed at the request of William S. Knudsen, who was then General Manager of Chevrolet. Like so many first attempts, the engine was unacceptable because of poor performance.

It was then decided to continue the diesel study with an entirely new approach. After these several years of preliminary study, it became apparent that the development of a light-weight, high-speed engine was no simple matter. A new program of development was proposed with these salient points. Obtaining controllable, clean combustion demanded a new fuel-injection system. To obtain high power output, it was decided that the engine should be of the two-cycle type. Two-cycle engines require an external air supply which became a problem in itself. High power outputs also imposed high loads on pistons, bearings, and lubricants. To obtain high speed, new and lighter valve mechanisms were required. At high engine speed, balance and torsional vibration presented new problems. To obtain light weight, it was necessary to develop new materials and methods of construction.

The program now established required that research work be applied to an engine. For the fundamental studies, a four-cycle,

single-cylinder engine of eight-inch bore and ten-inch stroke was built for the research group by the Winton Engine Company. They rated it at $37^1/_2$ horsepower. In addition, several other single-cylinder engines of smaller sizes were built by Research for studying the effect of size. However, our first successful engine came from the large single-cylinder Winton, which was dubbed "Big Bertha" by the researchers. (In engineering research and development on engines, it is usual to do the fundamental studies on a single cylinder to simplify the problem. The basic results are then applied to the design of a multicylinder engine.)

The engineering development of the engine was centered in our Power Plant Section. It is interesting to note that no one in the section had any previous diesel experience. The head had a background of work on aircraft and automobile engines. The assistant heads had similar experience, and the project engineers and designers were largely automotive engineers. Even the project mechanics who ran the engine in the dynamometer room had no previous experience with diesels.

Other departments of the Research Laboratories also contributed their specialized knowledge and experience. The Fuel Department, which had been responsible for the discovery of the effect of tetraethyl lead as a knock suppressor, had a strong background in engine combustion. Our Metallurgical Department was called upon to solve the new materials problems. Other groups were called upon to solve new problems in balance, torsional vibration, bearings, and lubrication. This was, in reality, a team or "task-force" effort without being called by these modern terms. Engineers, metallurgists, chemists, and other specialists each contributed in his own field. Independent studies of the many phases of the problem were carried out in various laboratories of the organization and the results brought together by the coordinating group.

The single cylinder "Big Bertha" was first operated as a four-cycle engine to establish a base of reference for subsequent developments. One of the first developments was a new two-cycle uniflow cylinder which was installed on "Big Bertha" with the

original crankcase and crankshaft. In this cylinder, the fresh air was admitted through ports in the lower end and the burned gases were expelled through valves at the top. The engine easily developed 80 horsepower with a clean exhaust—more than twice the output of the four-cycle cylinder.

The engine group was elated and realized that their studies and theories had been confirmed and that they had results of great potential value. This is what is now called a "breakthrough." But many further problems still remained to be solved before a successful engine could be designed. Here are 10 new major developments which made the General Motors diesel commercially successful, and there are many more:

Uniflow two-cycle cylinder
Unit injector
Helical lobed blower
Heat-dam pistons and materials
Oil-cooled pistons
Tin-plated piston rings
Hydraulic valve lash adjusters
Heavy-duty bearings
Engine balancing
Heavy-duty lubricants

I will describe only one of these developments, the unit injector. It was realized early that the key problem was the method of handling the small quantities of fuel for each power stroke. In one of our engineer's log books I found this statement: "Conference with Hallett and Kettering. Kettering stated that the major problem of diesel engines is the injection system."

From the preliminary studies it was determined that the fundamental requirements of a liquid-fuel injection system are:

(*1*) Accurate control of instantaneous rate of fuel injection in order to control combustion rate.

(*2*) Reduction of all hydraulic inertia and elasticity effects to the absolute minimum, making the injection depend upon pressure and resistance only.

(*3*) Mechanical operation of injector as free as possible from temperature, elastic deformation, and inertia effects.

A new injection system consisting of a combination pump and spray nozzle had been built experimentally by Winton, but the fuel pressures used were low. Results were unsatisfactory but encouraging when compared to the then-standard pump and spray nozzles. Taking this Winton system as a lead, a new high-pressure unit injector was developed. Pump pressures as high as 30,000 pounds per square inch forced the fuel through very small holes in the nozzle. This was many times the pressure that could be used on other types of pumps and injectors. The holes in the nozzle were held to six-thousandths of an inch in diameter. The fine spray this produces leads to good combustion without smoke in the exhaust. These high pressures were possible because there are no long high-pressure pipe lines between the pump and nozzle. Each cylinder has its own fuel pump and injector nozzle in a single unit, hence the term, "unit injector."

I have mentioned this development in some detail since it led to another problem—that of manufacturing the new unit injector. To obtain the high pressures, it was necessary to hold the clearance between the pump plunger and cylinder to 30 to 60 millionths of an inch. Several precisely located holes six thousandths of an inch in diameter had to be drilled in the injector tip. Very high-strength heat-treated materials had to be used to prevent splitting of the parts because of the high loads.

Manufacturers flatly declared it impossible to mass-produce a device to such tolerances. Research, therefore, took complete responsibility for developing materials and manufacturing techniques to produce the first unit injectors. In fact, for a number of years, Research designed and fabricated all unit injectors in a pilot plant having Research-designed tools and equipment. After it had been proved that these precise units could be produced

commercially, the entire operation was transferred to a production division. In several other cases, Research not only developed the part but also the method of fabrication. This is very unusual in our type of research, but it does show how far it is sometimes necessary to go to assure success of a research development.

Other technical developments which accompanied the work on the single-cylinder engine will not be discussed in detail in this presentation. A fundamental approach was taken on all problems relating to a complete diesel engine. The development of the new engine was not one problem, but a whole series of related problems, each one requiring a separate solution. Where needed basic information on a subject was not available, a new project was initiated.

Fundamental studies of air flow were made on two-cycle engine cylinders. The nature of the early program is suggested by the following contemporary statement: "Discussed the possibility of deriving a set of units to specify the resistance of a passage to air flow in terms of frictional resistance, 'capacity', and 'inductive' or inertia effects. This would make it possible to treat manifold and exhaust systems mathematically and compare different systems or units in a fundamental way."

Fuel-spray timing, penetration, and control were studied on bench fixtures outside the engine. Methods of controlling compression ignition by chemical additives to petroleum fuels became an active project which gave a better understanding of the diesel combustion process. The aim was the combustion of very lean mixtures, with particular reference to the constant compression cycle.

The charging-air requirements of a two-cycle engine led to a project on air blowers and pumps. As a result of this work, a new form of positive-displacement blower having spiral rotors was developed to supply a nonpulsating, smooth flow of air.

A basic study of heat flow in high-temperature pistons was undertaken to obtain information on materials, design,

cooling, and lubrication. Instrumentation was developed to measure piston temperatures in an operating engine.

From a research administrator's standpoint, the large number of problems that grow from a single project is no surprise. This multiplication of projects makes it extremely difficult to estimate costs and predict the time of completion. In this diesel engine development we had a very patient management, willing to continue supporting the project during the inevitable discouraging periods.

At the stage when many of the basic problems seemed to be solved, the Navy asked General Motors, through Winton, to bid on a 12-cylinder 900-horsepower engine to be installed in a mine sweeper. A 100-hour endurance run was a basic requirement to obtain the Navy contract. We were asked to run this test on our research single-cylinder engine. As in many such cases, it took three starts before the test was finally completed. The completion of this endurance test was the signal that provided the confidence and stimulus for further work.

As a rule, in our research organization we do not carry a development into the production stage. We obtain the basic information and build a working sample to demonstrate the principles to our production divisions. Research reports and technical memorandums are written and distributed to the divisions and executive groups to keep them informed as the program progresses. In this diesel-engine project, 54 comprehensive reports were written and distributed to other groups in General Motors who were interested in the progress of the research. Close contact was maintained with the Winton Engine Division, which had been acquired by General Motors during the early stages of this development work. The General Motors Truck and Coach Division, the Allison Division, and the passenger-car divisions were likewise interested and were kept informed.

The success of the research engine created such enthusiasm and optimism that General Motors management proposed construction of two eight-cylinder engines for supplying power

to the General Motors Building at the Chicago World's Fair. This installation would be a means of rapidly bringing the new diesel to the attention of potential users as well as obtaining firsthand operating experience on multicylinder engines.

Using the basic research information, Winton designed and built two eight-cylinder 600-horsepower engines which were one-fifth the weight and one-fourth the size of previous comparable diesels. They produced double the power from a given-sized cylinder, with very good fuel economy. The engines created much interest among the millions of visitors who saw them—including officials of railroads and other potential users of diesel power. Mr. Kettering often said that the biggest advantage of these engines for railroad locomotives was that they fitted inside a boxcar and allowed clearance through tunnels and bridges.

Inquiries and orders resulted from the public showing of the revolutionary new engines. The Navy ordered the first V-12 engines of this type for installation in a mine sweeper. This was soon followed by an order for V-16 900-horsepower engines for submarines. Four of these engines were installed in a submarine. These engines made it possible for the first time in history for a submarine to keep up with the fleet in surface maneuvers.

Many of the railroads were experimenting with new forms of streamlined trains. The newly-developed, two-cycle diesel proved to have just the weight, size, power, and fuel economy they were seeking. The first engines for railroad use were installed in the Burlington Pioneer Zephyr which set a record in a dawn-to-dusk run from Denver to Chicago, covering the 1,015 miles at a fuel cost of $14.88. It reached a top speed of 120 miles per hour.

With the new engine in production for marine, locomotive, and stationary applications, Research could now return to the further development of smaller engines which had temporarily been set aside. All the applicable principles of the large engine were incorporated in the smaller designs. Again, results were

encouraging, and several small-cylinder designs showed great promise.

This led the General Motors management to form a new group of development engineers to design a small production engine. This was a true "task force," including men from Research and several divisions. Research itself continued its broad studies of small-diesel problems.

The small engine soon demonstrated that it required a piston of greater strength than could be obtained with either aluminum or cast iron. Accordingly, an investigation was made of pearlitic malleable iron, a metal which had had only limited acceptance as an engineering material. Research metallurgists worked with our foundry division to develop the control and processing procedures for consistently producing this material which has properties intermediate between cast iron and steel. Pearlitic malleable iron is still used for the small diesel pistons.

In the meantime, the research staff had discovered the effect of trace elements in extending the size of castings that could be made from either malleable or pearlitic malleable iron. The first trace element found to have this effect was tellurium. Later it was discovered that bismuth or a combination of bismuth and boron produced a similar effect. Currently, pearlitic malleable iron with added bismuth and boron is used in a number of automotive applications.

The diesel-engine group used the fundamental information developed at Research to design a new series of three-, four-, and six-cylinder engines to cover a wide range of uses. As an outgrowth of their work, an entirely new organization—the Detroit Diesel Engine Division—was created to manufacture a new "71 Series" engine. These engines quickly found widespread use in trucks, buses, tractors, ice plants, cotton gins, pleasure boats, fishing craft, oil wells, and many other locations.

This, in brief, covers the development of two series of diesel engines from research to production. There were many unsuccessful experiments and false starts—as there are in any pioneer-

ing effort. Surely a less patient management could easily have found reason for dropping the project several times.

You might, as research administrators, be interested in what happened to the research personnel who contributed to the development. They were given the opportunity of following the diesels into the production divisions. Most of them chose this option and many hold key positions in engineering, production, and management. Almost 100 men were transferred from Research to the five diesel manufacturing divisions. For the research man who is interested in opportunities beyond those found in the research laboratory, this transfer policy offers further chance for advancement without leaving the parent organization. In General Motors it is an established policy that Research is a training activity for other technical phases of the business. Almost every other central-office staff or manufacturing division has research-trained personnel in its organization.

I will now sum up the results of this work as I see them, listing some facts which will serve as an evaluation of the project. General Motors now has five major divisions whose most important products came directly from the research projects on diesels. The Cleveland Diesel Division manufactures marine and stationary engines. The Electro-Motive Division produces diesel locomotives and diesel power cars. General Motors Diesel, Ltd., makes locomotives in Canada. The Detroit Diesel Engine Division manufactures small automotive diesels for hundreds of end uses. The Diesel Equipment Division makes unit injectors and other automotive parts. In addition, our General Motors Truck and Coach Division uses the engines in their trucks and buses. The Euclid Division uses the engines in their earth-moving equipment.

It is very difficult to evaluate fairly the end results of any research accomplishment. The effects may be so widespread that it becomes a major project in itself to obtain the facts. In this case, we know that diesel power was extended to hundreds of new uses in our civilian economy and in national-defense activities. According to figures released some time ago, 20,000 new

jobs were created in the five General Motors diesel divisions. These divisions have produced well over 100 million diesel horsepower, or about half as much as in all the central power stations in the United States. They have provided economical power plants for many businesses, large and small. A very important result is the conservation of our valuable petroleum resources through the use of the diesel's high thermal efficiency.

After the new diesel had proved commercially successful, one of our engineers who had played a key role in its development wrote the following: "In many respects, the engines developed by General Motors were not directly competitive with contemporary diesel engines. Due to their smaller size, lighter weight, and higher power, they could be used in many applications where existing engines were not suitable. Thus the economy and the reliability of the diesel engines were made available to many new users in many new types of machinery."

An important further result was the lasting effect on the future programs of General Motors and the Research Laboratories. Continued work on diesels has led to more highly developed engines. The work served as a good background for our present program on free-piston engines which begins to show great promise for many applications.

The two-cycle high-speed diesel engine pioneered the use of hydraulic lash adjusters on the valve mechanism. From this start have come the hydraulic lash adjusters now used on most automobile engines.

The diesel development accelerated the work on heavy-duty lubricants and high-output bearings. The results are, likewise, now applied to modern automobile engines.

We obtained an understanding of the structual problems involved in high-output engines and combustion processes at high pressures. This information became the basis for the development of the high-compression spark-ignition automobile engine now used universally.

New materials and processes were developed to solve many

problems. The material developed for diesel pistons found many other applications and is now produced in large tonnage.

An interesting side light was the proposal from a doctor to use the principle of the unit injector instead of a hypodermic needle. The very high pressures used in the unit injector gave a high-velocity spray which could penetrate the skin. It was his opinion that some medications could be injected by this method without the use of needles.

Last, but not least, there was the sense of accomplishment which everyone in the research group felt. It is a tremendous morale builder to see final success after several years of patient, often tedious, research in the laboratory. The development of a useful product which has meant progress not only to General Motors but also to countless users of this new power plant was a source of great personal satisfaction to everyone connected with the project.

IMPROVING THE PROFESSIONAL ENVIRONMENT OF RESEARCH PEOPLE: HUMAN RELATIONS ARE IMPORTANT*

R. P. DINSMORE

Vice President, Research and Development,
Goodyear Tire and Rubber Company, Akron, Ohio

Speeches and articles on the subject of dealing with the research scientist are, today, so frequently presented that the layman might easily form the impression that the subject of these attentions has characteristics somewhere between those of a prima donna and a juvenile delinquent. The causes of this intense interest are to be found in our pressing daily problems. These have been discussed so often that it should be sufficient to mention them without elaboration. These are:

1. Critical requirements of the military establishment for the perfection of weapons and counter-weapons of a highly technical nature.

2. The advancing automation of industry with its influence on the satisfaction of public demand for higher living standards.

3. The shortage of trained engineers and scientists required to support the military and civilian programs.

4. The growing threat of the communist bloc, accentuated

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by the Russian-forced program of scientific and technical training and the spectacular launching of a space satellite.

5. The steadily increasing cost of research and development accomplishment.

But why, then, is there such spectacular emphasis on research workers as people? Let us look at some of the factors that are frequently mentioned. The complaints of the research worker are the following.

a. He is not given the recognition to which he, as a member of a learned profession, is entitled.

b. His employer does not understand his skills and their possibilities, and, hence, the research worker is required to do too much subprofessional or nonprofessional work.

c. The management does not utilize his professional advice in planning the research program and in giving it a place in the plans for the future growth of the company. Rather, such plans are made as a result of the advice of nonprofessional people, with the expectation that a suitable research program can be fitted in afterward.

d. The employer expects to have research and development work done on a production basis.

e. Impossible demands are often made with respect to the time of completion of projects, while, on the other hand, there is a failure to utilize successful results which have been gained at the cost of valuable time and money.

f. The researcher is not given a sufficient chance to grow professionally and to enhance his own prestige.

g. Adequate opportunities for him do not exist within his sphere of professional activity; if he wishes the maximum advantages he must enter the administrative field.

The employer, on his part, makes the following complaints.

a. The researcher does not like to have detailed programs laid out for him and, when asked to do so, he frequently fails to prepare a satisfactory plan of his own.

b. Although he spends a great deal of money, it is difficult to understand his explanation of what he has done with it.

c. He is inclined to spend too much time and money on certain pet projects that never seem to work out.

d. He would like to be treated like a doctor or a lawyer, but he does not like to accept equivalent responsibility. Very often, he does not have the ability or the patience to explain things clearly to his employer the way a doctor or lawyer would do.

e. Unless the research worker is under personal observation by the employer, or there is a rather extensive record of his work and his attitude, the employer finds it difficult to decide whether he is a serious professional man or merely a routine technical worker. The employer believes that he cannot rely upon the academic degree, the length of experience, or professional society connections. The employer would like to encourage and reward creative ability and responsible project planning, but he is confused as to how to go about it, especially when it comes to the selection of scientific people in the earlier stages of their careers.

The public reaction is more difficult to evaluate because it is more diffuse, and those representatives of the public that are most vocal do not necessarily represent the largest numbers of people. However, there seem to be certain reactions that are fairly general. Some of these will be given here, but not necessarily in their order of importance because I am not sure that I know how to weigh them properly.

a. The more important scientists are not sociable—they don't like to mix with people, but prefer to read scientific books and work on difficult problems.

b. They are materialistic, anti-religious, and frequently feel a greater allegiance to science than they do even to their own country. In other words, they are socially and politically unreliable.

c. They get an exceptional, almost inhuman, pleasure from hard work, as compared to participation in sports or ordinary pleasures.

d. They are essential to the accomplishment of difficult tasks required by both the military and industrial producers, but the ordinary man cannot understand what they are doing or what they talk about; as a result, they are respected for their ability but are generally unpopular.

It is not my intention to go over the same ground and repeat the same arguments that have been heard again and again. This would not accomplish anything useful. The reader, with his scientific background, can detect without difficulty most of the errors contained in the various viewpoints that I have outlined. It is, perhaps, not quite so simple to determine the degree of deviation from the truth or to explain in readily acceptable language the reasons that many of these distorted views exist.

The arts have always preceded the sciences. People have learned to do many things extremely well before they obtained enough information to understand the reasons underlying the successful methods. I mention this because organized research is relatively new, and, even though the ranks of competent research workers are not being augmented as rapidly as we would like, they have, nevertheless, expanded rather more rapidly than our understanding of the underlying personal motivations. Thus, it may be that, even though we as scientific people like to have scientific explanations for the things we are doing, we may have to accept for a while the results of beneficial experience which can be rationalized later after there is enough of it at hand.

Perhaps one of the most perplexing phases of the situation we are discussing has already been touched upon, namely, that there are all degrees and gradations of scientific ability which do not rest entirely upon training and experience. These differences are due in part and, sometimes, to a preponderant degree, to individual aptitudes and such other personal characteristics as energy, optimism, enthusiasm and persistence. This is disturbing, not only to the employer or the manager of research people, but also to the people themselves. Frustration is apt to come to the individual to whom are assigned tasks he cannot complete or can

complete only imperfectly, while, at the same time, he sees others do them quickly and well.

I would like to pause here and observe that this kind of frustration can affect people with widely different abilities. The skilled routine physicist or chemist who can follow a given procedure with estimable precision may be greatly disturbed if he is assigned the task of creating new methods for which he has no aptitude or interest. On the other hand, the creative scientist who is bubbling over with new ideas may, if tied down to working them through in tedious detail to some finished application, be completely frustrated because he does not have the manual dexterity and patience required to duplicate the skillful procedure of the more routine worker. Many other illustrations will occur to each of you as you think about this. There is the one so frequently found in research organizations: that people who are excellent scientists and can produce very valuable results from their own minds and hands are absolutely lost when it comes to accepting the responsibility for directing and coordinating the work of others.

It is very likely for these and other compelling reasons that a number of organizations, both large and small, have instituted screening tests, some of which are psychological in nature and others which have to do with technical competence and experience. However, my own feeling is that any screening test, unless it is of a very general nature, is unbalanced and tends to be misleading. In any event, even after proper classification, a major problem remains, namely, how to balance the efforts of research workers with various aptitudes and combinations of aptitudes.

When it comes to dealing with research people, it seems to me that the first misconception to avoid is that they differ in any essential way in their personality characteristics from other people. It is true that the specialized training that they have acquired indicates that they are in an upper level of intelligence. It is also true that our scientific and technical education has tended to neglect some of the broadening influences that are to be found

in a study of the liberal arts. I suppose that this might be simplified, perhaps oversimplified, by saying that the scientist has gained an unusual understanding of the systematic nature of natural phenomena and perhaps somewhat inferior knowledge of human and social relationships, either as compared to the liberal arts graduate of comparable mental caliber or the intelligent person lacking formal training but having extensive human contacts.

Despite the human tendency to place all the blame for disturbed relationships on the other fellow, there seems to be very little inclination to believe that either the research worker or the industrial employer could get along without the other. To be sure, some scientists engage in private consultation and research, but they constitute a very small minority. Also, there are many industrial concerns that do not directly employ research workers, but more and more of these are obtaining research aid from industrial research laboratories, independent laboratories or, at the very minimum, as a service, from their suppliers' laboratories. Moreover, the public does not believe that research workers are expendable either, although it sometimes expresses a preference for Government control of research—especially of the more fundamental variety.

The rapid increase in the size and number of large research laboratories is removing the problem from that of the individual creative worker or the small group composed of those with similar aptitudes and congenial temperaments. Instead we have, increasingly, large heterogeneous groups working on a variety of complex problems and large, annual expenditures of money which must be justified by results.

It is natural that methods of research management have varied widely, because, as in wider aspects of industry, forceful and successful leaders have instituted various methods, many of which lose their significance in the absence of their particular advocates. For example, successful management has been said by its participants to be attributable all the way from "the best

research management is no management" to "research can be planned, costed, and directed exactly like any other phase of business."

As is ordinarily true in changing social practices, patterns emerge slowly at first and then take shape more rapidly. When these changes involve segments of society with differing backgrounds and work methods, the pattern evolves more slowly. A case in point is the conflict between capitalistic management and union labor in their search for a method of attaining economic success and agreement on distribution of rewards according to the contribution of each, while still providing an equitable return to the stockholders. Fortunately, not so great a disparity exists between the views of the research worker and those of his employer; however, some of the important differences between them result from similar causes. For example, many of the reactions of the union worker are believed to arise from loss of his individuality as a producer. The research worker's complaint that he is not treated as a professional person comes from a similar frustration of his wish for personal prestige. It would not be profitable, however, to continue the parallel. There has been no answer to either problem, but it would seem that enduring solutions of both problems must recognize the necessity for an economic balance on the one hand and satisfaction of the desire for individual recognition on the other.

Many managers believe that the successful handling of any group of people depends primarily on personal acceptance and liking of the leader by the individuals in his group. Although this factor is truly important, I believe that it is far from being able to succeed alone.

In any job, it is essential that there be a clear understanding as to the nature of the work and the time and money that is allowed for its completion. It is necessary that the manager be sure that the worker has the skill to carry out the work or that methods are available by which he can reasonably acquire it. The more the exercise of judgment required of the worker, the more complete

must be the information given him regarding the purposes and requirements. If the job is a long one with several stages, there must be methods by which it may be reappraised at necessary intervals. All of this requires good communication back and forth—a prime responsibility of the manager. Obviously, when the individual judgment of the professional scientist is involved, the information required is greater and must be adequate if sound results are to be expected. This is an obligation on the part of the management of the company, the research management, and the professional scientist involved. All must recognize that adequate information must precede a professional job.

If a manager assigns a job to a worker which the worker cannot perform because he is lacking in either skill or experience, the manager is inviting failure. Likewise, if the professional worker accepts the assignment, he is violating professional ethics and undermining his future. Therefore, the manager must know the capabilities of his people and must properly utilize their skills. At the same time he should provide the young professional man with the means to add to his knowledge and experience and the opportunity to exercise judgment within his capacity. The professional, on the other hand, should not expect to gain professional stature in one jump. He must accept his own limitations as applied to immediate problems and work earnestly for added knowledge and experience. He should study his company's activities and policies in order to build a firm platform for future planning and recommendations.

The professional man finds a rich source of knowledge about technical advances and professional practices in the meetings of his professional society. Here, he has a chance to become acquainted with an important cross section of his colleagues and to gain recognition of his own worth and ability. He should cultivate this fertile field, and his employer should encourage him and help him to do so. The employee should not expect encouragement unless he displays an active interest, and the employer should not expect the employee to assume all expense such as that of meeting

attendance. At least at intervals, the employer should pay the meeting expenses of the employee who has shown desire for professional development and enough interest to pay dues and prepare papers.

The manager should arrange assignments so that older professional men can work with and help develop the proficiency of their younger colleagues. Adequate technicians should be available to aid the professional and to enable him to give full attention to the professional aspects of the job. Likewise, the older and younger professionals must recognize their obligation to implement this process.

There are researchers who combine creative thinking with laboratory dexterity and ingenuity in developing and perfecting the laboratory techniques necessary to carrying the new idea through its early stages, where theory is either confirmed or denied by practice. Indeed, some must follow through to final success or failure, however far that may be, before being satisfied to give full attention to a new project. The skillful manager will work out the steps of the project with an eye to utilizing, to the greatest degree, the talents and the interests of his staff.

The functioning of groups or teams can be good or bad, according to whether or not the elementary requirements are recognized. Most managers understand that team effort depends upon balanced capacities, coordinated and directed toward a common objective. Each member must understand and perform his own duties and, at the same time, have confidence that all the other members will do likewise. However, the fact that each individual has personal aims or ambitions which aid or hinder the team project, depending upon whether it is compatible with his (long-term) aims, is not always considered important. Thus, if an individual has managerial aspirations, he will tend to welcome association with a project that offers him some experience in handling people or an opportunity to gain broader insight into the workings of the company's organization, and he will view with disfavor a project which does not offer such training. Similar

reactions occur for other personal objectives, whether they be to broaden professional experience or to acquire skill in plant operation. The individual's personal aims, if known, can be used to reinforce and improve team operation.

How, then, can professional individuality be reconciled with the desire of management to work out a program which closely follows the company's planned pattern and which has a high degree of probability of economic success?

First, the planned pattern must be known and, moreover, analyzed for weak spots or deficiencies which might be corrected by successful scientific research. Present problems are easily understood; those several years away require more vision and judgment. Also, the research director will recognize the need for some fundamental research, as both a basis for meeting future needs and a means of attracting and keeping superior scientists.

The second requirement is that the research director lay out a program which will reasonably care for the company's needs and that he estimate the time required for completion of the various proposed projects, their respective costs, and the rate of total annual expenditure. It is at this point that many programs run into trouble, either because the cost is considered too high or because the director is asked to give too definite guarantees of performance. Especially if there have been recent project failures or serious expense over-runs, the management looks for such assurances which the director fears to supply. Such a situation can deteriorate rapidly, to the detriment of management relations, and lead to feelings of distrust, discouragement, and frustration on the part of the individual research worker. The fault may lie largely on the side of management as a result of unreasonable expectations, instability of plans on which research requirements were based, or inadequate communication, or the fault may have originated with the organization or operation of the research laboratory itself. Whatever the reason, it is largely up to the research director to correct the condition. This is because only he can talk both the language of the scientist and the language of

management. He must find ways to translate his activities into language which management can understand and evaluate. This calls for a careful analysis of each problem, designed experiments which can be rated numerically as to probable success, and cost and time estimates which are based upon experience meticulously applied.

The result of such methods will strengthen the research laboratory's prestige and bridge the gap between the business management and the scientist. Strangely enough, this is not only good for the management and the business, it is also good for the research worker. He is more frequently assigned to the kind of job he enjoys and can do well; he has a higher percentage of successes; and he suddenly finds that he and his associates have gained stature in the organization.

So we return to the mutual criticisms of management and research workers and the somewhat distorted light in which the latter stand in the eyes of the public. All concerned have normal, human characteristics, including suspicions and antagonisms directed at the things they don't understand. Perhaps the scientist suffers most because his language is the most obscure to others.

The solution of most of these problems lies in understanding the human attitudes involved and not in defeating but in utilizing these motivating forces to accomplish harmony and success. The greatest burden of this task falls upon the research director. He and his supervision must know and consider the individual researcher, his aptitudes, his experience, his aims, and his attitudes, just as the scientist must know the composition of the materials with which he deals and the energy changes involved in their modification. He must utilize this knowledge to balance his project teams. When this has been accomplished, he will find it relatively easy to encourage and aid his staff in additional activities which will enhance the prestige of both the organization and the individual and which will add to the confidence and sense of achievement of his scientists.

It may seem unfair to the research director that he should be expected to adopt management schemes that include estimating, accounting, and statistical design, but he will find that these languages bring him rich rewards in his own sense of achievement and his vastly improved relationships with an understanding management and a cooperative research group.

SALARY ADMINISTRATION OF RESEARCH PERSONNEL*

GEORGE L. ROYER

Administrative Assistant to General Manager, Research Division, American Cyanamid Company, New York, New York

This paper on salary administration of research personnel has certain characteristics of the hybrid, for in it I have written about both general and specific aspects of research salary administration. In going from the general to the specific, the approach to the field of research management which we are developing at the American Cyanamid Company has been outlined. This is followed by a discussion of the general field of salary administration, after which details on salary administration in the Research Division of Cyanamid are presented. I want to stress the words are developing in discussing our work in this field, for, although we feel we have made strides, we are not completely satisfied that we have the final answers to the problems of financial compensation. Like any art or science, this development is a continuous process, all aimed at finding the optimum means of stimulating, recognizing, and rewarding creative and productive effort.

With the formation of the Research Division of Cyanamid several years ago, all Research and Development personnel of the company became the responsibility of this Division. Professional personnel at the three laboratories at Bound Brook, New Jersey; Pearl River, New York; and Stamford, Connecticut, number

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over 900 and, with other auxiliary personnel, the total personnel of the Division is about 1,950. Our total Research and Development budget in 1957 was about $23 million.

Before going into the history of our research salary administration work at Cyanamid, it seems appropriate to begin by exploring, in a general way, the basic philosophy and outlook which is inherent to our approach to research. We felt from the very beginning that research management (including salary administration) was not the same as managing a production enterprise. We also felt that the classic approach to salary administration was linked to the fact that industrial management and organization usually has its basic logic or structure largely determined or controlled by production machinery and processes. These hard-core tasks thus establish the foundation of the organization almost automatically. We in the Research Division had no such structure, so we were relatively free in forming our organization.

Not many of us who have spent any length of time delving into the field are convinced that there is any one best answer on how to organize the research function in general. It seems to me that here we have been strongly tempted to perpetuate the traditional production-management approach. This is a sizable topic in itself and one which is receiving more and more attention today. Certainly, we are beginning to realize what we do not know, and this is perhaps the beginning of wisdom. Such books as *Human Relations in Industrial Research Management*, published by Columbia University Press last year, are highly indicative of the probing and reassessment which is going on in this field of research organization and management.

In their introductory chapter, the authors make some extremely pertinent remarks concerning a central problem of research management when they say:

"What knowledge is available about . . . research organizations and classes of behaviors . . . of research groups comes principally from descriptions of operating structures . . . cast as statements of 'functions' and formal authority relationships, such as those purported to be shown by organization charts, job descriptions,

and policy statements. The difficulty with such pictures of organizations is that they do not represent ongoing relationships and dynamic value systems, and it is impossible, without independent investigation, to determine their correspondence with the actual organizational process. They may be useful as an adjunct to administration, but they are inevitably static . . ."

Obviously, one cannot separate the formulation of an equitable approach to research salary administration from the very nature of the research task itself, and, in that respect, the same authors offer the following cogent observations on research and creativity:

"Research is conducted by combining resources of all kinds within cycles of experimentation, thought, observation, and conceptualization. But elaborate facilities, libraries, and intelligent, even highly trained, persons alone cannot produce results. The research job involves a process of identifying, locating, and using these physical and intellectual materials in a manner that cannot be rigidly prescribed, except in a very general way. The individual must bring a unique quality to research, a quality about which almost nothing is known . . . Research . . . is expected to require creativity, invention, or ingenuity."

This tantalizing, hard-to-isolate (and harder to define) problem of the nature of creativity is certainly at the heart of the problem of compensation in research. How can the creative scientist and management reach an accord on what is fair compensation? Part of the difficulty of answering this lies in the nature of research today. It is becoming more and more difficult for even the most gifted industrial research scientist to be creative without a formidable array of total facilities. This would encompass not only suitable physical surroundings and equipment but also a variety of associates with various knowledges and skills who may stimulate or otherwise contribute to the intricate creative process.

The first questions we asked ourselves about salary administration were, "What are the different factors and assumptions which should be questioned in approaching the problem of research salary administration?" and, concurrently, "What portions of the classic business approach to job classification and salary administration should be retained, and what new features might

be required in order to give us the kind of salary system best suited to our own needs and acceptable to corporate management?"

It seemed to us that the traditional concepts and techniques of salary administration have evolved quite naturally from the early work which was done in job analysis, particularly as it relates to manufacturing and production operations. It also seemed entirely natural and logical that such traditional systems would and did reflect the approach of the time-study man and the industrial engineer. As you know, this approach places heavy emphasis and insistence on "the separation of the job from the man." Under this concept, any one job is viewed as a relatively rigid series of tasks to be accomplished, and the amount of compensation to be paid for each job is decided on the basis of point comparison, ranking, or other means of aligning the jobs in a stratified structure.

Among the more important characteristics of such a salary structure we find:

(*a*) Job descriptions, in which all jobs are described and classified in detail.

(*b*) Job evaluation, in which each job is "rated" and the wage structure is tied to this rating.

(*c*) The wage structure is elongated to encompass all jobs and may have narrow limits for each range within the wage structure.

(*d*) Advancement or growth in title and salary must be achieved by moving from job to job.

The above is, of course, a simplification of an extremely complex system, but, in the main, it can be said that, under this system, the job hierarchy dominates, and men must move from job to job within this hierarchy to achieve real progress.

Some may question the need for any kind of formal compensation structure or program. To do without formal structure or program would seem to be a most difficult course to pursue, particularly in a large organization, for without some formal basis or

structure of comparison, compensation would evolve into chaos, with the individual and management operating more and more on an individual and unorganized basis.

The problem of compensation of research efforts is further complicated by the difficulty of placing the credit for most scientific developments squarely on the shoulders of any one individual. As those of us who have worked in the field for any length of time know, there are always members of an organization who may be catalyzers of productivity and creativity, although they lack the final interest or skill required to bring it to fruition. This fact highlights some of the problems involved in the area of compensation for which some form of bonus or incentive reward may be desirable.

These problems are germane to the approach to research salary administration, and their answers, which are very elusive, point to the need for some classifiable method of judging compensation. The approach should have within it not only the possibility of fairly rewarding the outstanding performer, but also of assuring others whose labors are less obviously productive but whose work is of true import to the total task that they, too, are being properly and fairly compensated.

To return to Cyanamid's specific practices, we decided from the very start that in research the job should not dominate the man, but that, rather, the man does and must make the job. It seemed to us that one way to approach this problem would be to ask ourselves, "What kind of men do we have?" and "How have they developed?" In other words, if we could remove the blinders and conditions that influence our thinking about the man-job relationship in research, perhaps we could come up with a simpler and more valid classification system than is generally used.

In approaching this, we focused, originally, not so much on salary as on attempting to define certain levels of professional competence. This task was made somewhat easier by the fact that, in research, perhaps more than in any other field, there is a relationship, at the bottom of the scale, between education and

competence. In other words, it is not common practice to introduce anyone to the research field at the professional level unless he has at least a Bachelor's degree. So far, no one has developed any means of predicting the latent ability of professional personnel in terms other than what we know of the quality of their education and experience. For this reason, up to this time, these are still the only generally accepted criteria for establishing salaries for new personnel. Starting salaries for B.S., M.S., and Ph.D. scientific graduates are no secret today. The young scientist is usually interviewed by a number of potential employers, and an informal but effective communication system at most universities allows the new graduate to conduct his own survey of starting salaries. Industry, for its part, keeps in touch with the universities and moves toward uniformity of salary in competing for graduates. Offers are generally in line within industry in order that each company may attract its share of new talent.

Discussion within our research organization led us to believe that we could divide our scientists into three areas of growth or development of competence which might be designated by different titles. For salary-administration purposes, it would be possible to divide each of these into two levels, so that men of different ability could be compensated properly in relation to their rate of growth.

The B.S. graduate enters the Cyanamid organization as Scientist, *i.e.*, chemist, physicist, biologist, etc., and thus our Level 1 is established. The establishment of criteria for this and the higher levels involved a series of discussions and conferences within research management. These delineate such factors as degree of supervision needed or given, special training requirements, versatility, and technical competence. (See Appendices A and B, which are discussed below, for examples.) Level 2 is the level at which the M.S. graduate without experience can enter the organization. It is also the level which the outstanding B.S. graduate may reach after a minimum of one year's experience. The Ph.D. enters the organization near the top of Level 3 and is

classified as a Research Scientist. The outstanding B.S. who meets the total criteria requirements can reach this level after a minimum of five years' experience; an outstanding M.S., after a minimum of three years' experience after graduation. To move into Level 4, an outstanding Ph.D. requires one year of experience. He may move into Level 5 as a Senior Research Scientist at a minimum of five years after receiving his Ph.D. and into Level 6 and above, if qualified, as will be described later.

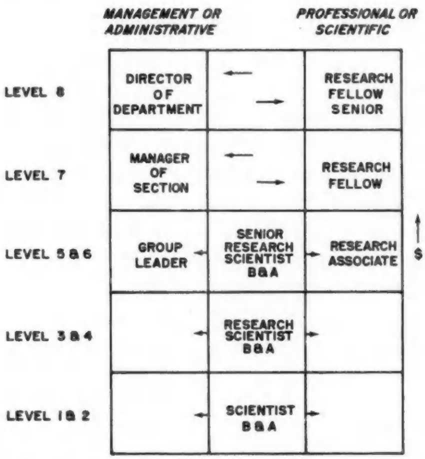

Fig. 1. American Cyanamid Company Research Division Development Program.

It should be noted that the coordinated pattern of experience and progress described above is not so rigid as to prevent the truly exceptional contributor from receiving fitting recognition and rewards. It is possible, in such cases, to move outside the system or to move within the system at a faster rate.

The levels and titles which we have developed are shown in Figure 1. Let me restate one of our basic concepts, *i.e.*, a man's salary is related to his research and/or administrative competence; our scientists, as they grow in competence, can move up from level

to level without changing the areas of their jobs. The description which I have given of moving from one level to another on the basis of competence and years of experience may be misleading unless you can visualize the salary range of these levels. The levels have a 40–50% range from the minimum to the maximum. As you can readily see, this provides ample room for a man to receive merit increases as he is developing within such a range. The men are not hemmed in by a series of narrow job descriptions. In a sense, they can carry their jobs upward with them in their professional growth. The developing scientist may "grow like a tree," with his roots spreading ever deeper into his chosen area of professional interest.

If you examine Figure 1, you will note that, at Level 5 and above, the developing scientist has a choice of two avenues to further growth and higher compensation. Our basic aim here has been to eliminate that situation in which scientists have to move into the field of management in order to continue to gain higher compensation. The concept that a scientist, working as a scientist, can make fully as great a contribution as is made by a supervisor is, I believe, receiving more and more acceptance by research managements. In short, we seem to be leaving behind the era in which many scientists were forced into the field of administration to get more money and recognition.

The mere establishment of such basic concepts as those I have described is, of course, a long way from a total solution to the problems of salary administration for research. The next question is, "What constitutes fair compensation?" So far, even in this age of Univac and Sputnik, no one has yet come up with a scientific means of determining what constitutes real equity in salary compensation. One reason for this is that no corporate management can exist in a vacuum—even if it were possible to evolve a unique and completely valid system, it would have to meet the competitive situation and the demands of the current market place for the talents involved.

Our policy, like that of most major corporations, is to pay the kind of competitive salaries necessary to attract and retain

our share of the most capable personnel in American science. To do this, it is necessary to know how our rates compare with those of firms in competition with us for research personnel. Therefore, we participate with many companies in surveys and studies of salary rates and trends.

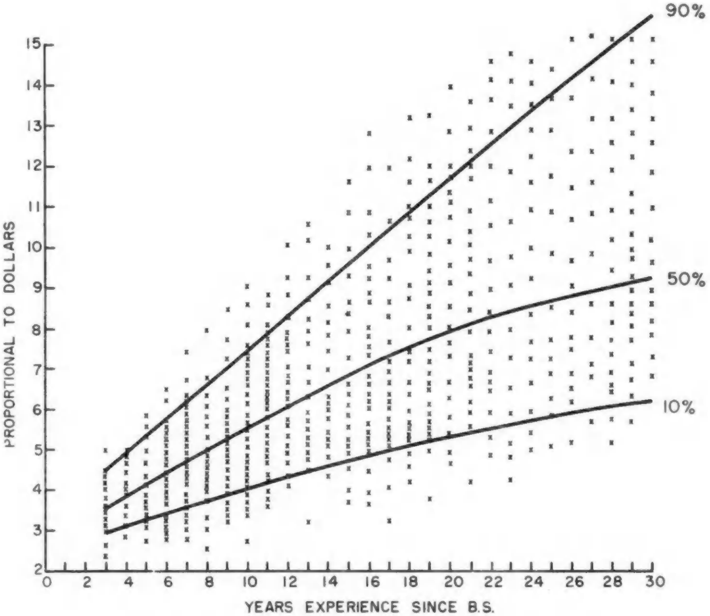

Fig. 2. Hypothetical scatter diagram of Ph.D. salaries.

We have found that it is very difficult to rely on job titles, or even job descriptions, in attempting to set up standards of comparison among the various companies. Thus, over recent years, we have come to rely more and more in our external surveys on the use of scatter diagrams of data, in which the compensation rates for research personnel in these companies are plotted on a chart

against the number of years of experience. A chart of hypothetical data for Ph.D.'s is shown in Figure 2. Separate charts are made up for B.S. and M.S. graduates. We have found this salary-experience relationship to be fundamental in making comparisons with outside firms.

We use our outside-survey data to check the salary ranges for each of the levels of the Professional Development Program.

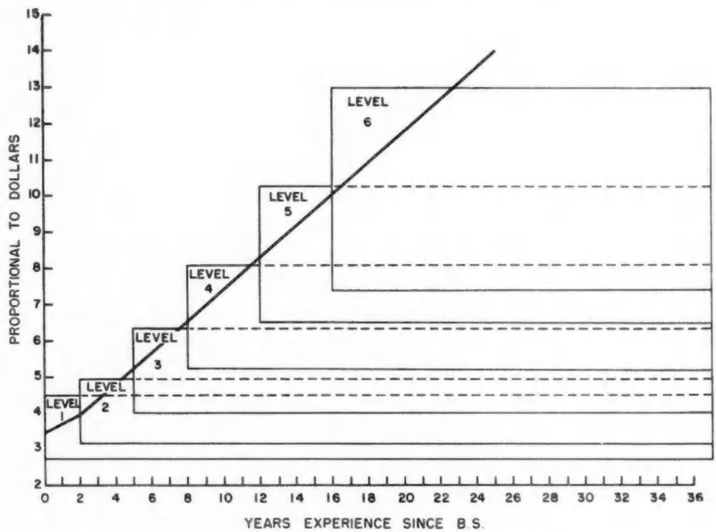

Fig. 3. Hypothetical reseach salary levels.

Curves are drawn at the 90 percentile, 50 percentile, and 10 percentile levels (see Fig. 2); the 90 percentile curve for Ph.D.'s in the chemical industry is chosen as a guide in setting up the range maxima. Each level has minimum requirements with respect to years of experience, and the maximum salary limits for each range are established with the aid of the 90 percentile curve. Thus, the most outstanding men (upper 10%) can be properly

compensated within the salary-level structure for our Professional Development Program. Figure 3 shows graphically a hypothetical salary structure based on the data shown in Figure 2. By this procedure we have been able to establish our levels and correlate them with salary data obtained on the basis of years of experience.

Internally, we attempt to keep our classifications and salary structures consistent with those of other parts of the company by a process of review and appraisal. We are aided in this by the Corporate Personnel Relations Salary Administration group, which helps us make comparisons with other types of work within the company, especially those at the managerial level.

Each man's place in the Professional Development Program is determined by a comparison of his knowledge and skills with the established requirements for the various levels. Appendices A and B, referred to above, are given as examples of the established requirements for Levels 1 and 5, respectively. These are excerpts from the specifications check list which defines the normal minimum qualifications and duties for each level of scientific personnel in the Research Division. It is recognized that exceptions will have to be made occasionally for outstanding scientists, but the specifications are expected to cover methods of progression for most of our scientists who fall within those classifications.

The major difference between each of the succeeding levels lies in the increasing skill, degree, and scope of responsibility, or in the increasing degree of independent effort required. Although, with increasing maturity and experience, good growth is to be expected, accumulated acceptable service alone is not sufficient to justify any specific level or change in level. Advance is contingent upon demonstration of ability to fill the requirements of the next level and evidence of the kind of personal contribution which distinguishes the scientific worker from the laboratorian, scientific assistant, or operator.

Certain terms employed in these specifications have been defined, such as problem, project, program, field, area, supervision,

and degree, so that relationships can be established from level to level. Certain general requirements are basic to all levels; without meeting these requirements, no individual would be considered for filling any professional level. These are:

1. Ability to cooperate effectively with co-workers.
2. A satisfactory work output.
3. Scientific and personal integrity.
4. Ability to perform duties of any lower professional classification in his area.
5. A professional attitude, expressed by interest in professional societies, the desire to experiment, curiosity, and a critical attitude.

The performance of every man is subject to review at any time (but not less than once a year) to determine whether he has developed to the next higher level. At the salary review period, comparisons are made among personnel of the same level, and salary increases are given within the ranges of the level, depending upon the results of the comparison. The better men will move faster through the range than the less qualified and, naturally, will receive larger percentage increases. Administration is under the guidance of a salary administrator at each of our research laboratories, but the actual comparisons and recommendations are made by line organization personnel. Our first line of supervision, the group leader, appraises his men in each level and then discusses these appraisals with his supervisor, the manager. The manager discusses these ratings with the director of the department. Finally, all of the men in the laboratory in each level are ranked in regard to their performance. At the manager level and above, comparisons are made across the Division. Our evaluation program has been discussed in more detail elsewhere. Adjustments in dollars varying with the performance are made in relation to the over-all salary budget for the location. In establishing this over-all salary budget, we consider the amounts needed for merit increases and to maintain a competitive condition.

Perhaps it would be appropriate, in concluding, to make some general observations about the relationship of proper compensation to over-all morale and productivity. By now, most of us, I am sure, are in agreement that men do not live by bread alone. Beyond the certitude of this Biblical wisdom, we have the evidence offered by many surveys of those things scientists value most in appraising "a good job." Great emphasis is placed on the opportunity for interesting, worthwhile, and challenging work in an environment where scientific accomplishments are given full recognition. Essential are the opportunities to publish and to continue to develop professionally through further study and scientific society work.

Seen in perspective, the role of the scientist in our culture has undergone—and is still undergoing—an extremely rapid metamorphosis. In fact, it is difficult today to read any leading periodical without coming across some article in which an attempt is made to analyze and evaluate the scientist's proper role in society. The great demand for scientists in every field has produced dislocations in scientific and economic status, and the recognition and demand for both pure and applied research has caused many a scientist who never planned such a move to leave the academic world and move into industry. On every hand, today, securely placed scientists see and hear of opportunities which seem both challenging and profitable. It is a period of such change and transition that it is hard for the individual to escape the feeling of restlessness. In such a time of transition, obviously, all the problems of morale cannot be solved by better compensation alone.

The administrator of research who, among other things, has the responsibility for salary administration is trying his utmost to understand all that is going on in this area today. Most research administrators have spent years as research scientists before moving into management. They are cognizant of the kind of analysis and understanding which is needed in order to bridge some of the gaps which naturally exist between research

personnel and management. The scientist, enmeshed in this involved situation as he is, can do much to speed and ease this task of integration, as he better understands the various forces which are at work in his segment of society today. This includes, of course, an effort to understand the needs of management.

Most men in research management today feel that their major objective is to attempt to meet these issues squarely and, in as scientific and objective a fashion as possible, work for the best welfare of all concerned.

Acknowledgment is made to the many people in the Research Division and elsewhere in the American Cyanamid Company who have contributed their time and thoughts to the various aspects of our Professional Development and Salary Administration Programs. Appreciation is also expressed to Philip R. Kelly of our Central Personnel Relations Department for his help in the preparation of this paper.

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Appendix A

Specifications for Scientist B (*Level* 1)

Educational and Experience Requirements:

A Bachelor's Degree with no qualifying experience; or

Scientific or technical study and experience equivalent to a four-year college scientific or technical training course.

Function and Supervision:

Understands and works effectively under sustained supervision on technical problems. May train, develop, counsel, and guide individuals of lower classification.

Performance Requirements:

Has general knowledge of science *or* has ability to apply simple principles and techniques.

Reads and comprehends suggested literature.

Reaches simple scientific conclusions. Makes useful suggestions on minor points of his problem.

Records and collates data adequately.

Recognizes changes from the normal. Makes independent decisions on minor points of his problem.

Appendix B

Specifications for Senior Research Scientist B (*Level* 5)

Educational and Experience Requirements:

A Ph.D. Degree with a minimum of five years of qualifying experience; or

Some years of experience as Research Scientist A.

In rare and unusual cases an individual without these formal educational requirements may qualify because of extensive specialized experience in a particular field.

Function and Supervision:

Understands and works effectively under limited supervision on programs. Plans and schedules work under sustained supervision on programs, and under limited supervision on projects. May direct work under limited supervision on projects; must direct work if in an administrative supervisory position.

Special Requirements:

Has demonstrated ability to coordinate several problems simultaneously, to deal effectively with unusual situations, and to attain difficult objectives. Recognizes the potential scientific or commercial value of scientific developments.

Special Requirements Applying to Administrative Positions Only:

As an administrator, the same as Research Scientist A but has broader responsibilities and authority, *e.g.*, the position frequently involves supervision of two or more Research Scientists B or higher, in addition to necessary personnel having a lower classification.

Performance Requirements:

Has extensive knowledge in his area, or demonstrated ability to acquire such knowledge rapidly in a new area; has general knowledge of related areas.

Reviews the developments in his area continuously, interprets them and makes sound recommendations.

Through original suggestions, contributes significantly to the projects in his area and would be expected to do similarly within a reasonably short time if reassigned to a different area.

Communicates effectively with others outside his area.

Acts as a sounding board for testing scientific hypotheses and as a stimulus for the attainment of the highest caliber of scientific thinking and experimentation.

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